

LETTERS TO THE EDITOR.

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The Function of Reissner's Fibre and the Ependymal Groove.

THANKS mainly to the investigations of Porter E. Sargent, Reissner's fibre is now known to occur throughout the vertebrate series from the lamprey upwards. Not long ago Mr. G. E. Nicholls directed attention in these columns to its occurrence in the frog, and Sir Victor Horsley (*Brain*, vol. xxxi., 1908) has recently shown that it occurs in monkeys (*Macacus*). I have myself already described it in *Geotria*, and have lately observed it in the tuatara and in the cat. Its position and relations are remarkably constant. Commencing beneath the posterior commissure, it extends backwards to the hinder extremity of the spinal cord, lying, at any rate for the greater part of its length, quite free in the iter, the fourth ventricle, and the *canalis centralis*. Anteriorly it breaks up into very slender fibrils, which are attached to the columnar epithelium of the ependymal groove.

The ependymal groove, though its existence has been strangely ignored by most observers, also occurs throughout the vertebrate series from cyclostomes to mammals. It is found typically as a deep longitudinal furrow on the ventral aspect of the posterior commissure, lined by high columnar epithelium, very different in appearance from that which lines the greater part of the brain-cavity. In this highly specialised condition I have observed it in the lamprey, the tuatara, and the cat. I suggested some years ago, in the case of the lamprey, before the connection of this structure with Reissner's fibre was known, that it might aid in the circulation of the cerebro-spinal fluid, by means of cilia which I thought I had detected on the epithelial cells. Whether this be so or not, I now think that the ependymal groove may have another, and perhaps more important function as an intra-cerebral sense-organ.

Sargent, as is well known, has interpreted Reissner's fibre as a nervous structure which serves for the "short-circuiting" of motor (optic) reflexes, and he regards the epithelium of the ependymal groove merely as an attachment plate for the fibre. This seems to me hardly a sufficient explanation of the existence of such a highly differentiated organ.

The nervous nature of Reissner's fibre is very far from having been demonstrated, and though I was formerly led to agree with Sargent's opinion on this subject, I can do so no longer. Sir Victor Horsley and Dr. McNalty (*loc. cit.*) have shown that electrolytic lesions do not cause any degenerative changes in Reissner's fibre such as would be expected if it were a nerve-bundle, and this result is, of course, totally opposed to that of Sargent's earlier experiments, by which he endeavoured to establish the truth of the optic reflex theory. On the other hand, there is a certain amount of evidence to show that Reissner's fibre is highly elastic, and therefore more of the nature of connective tissue. Thus it is often found that when the spinal cord has been cut across, the fibre has "sprung" and twisted itself into a knot or gnarl in some part of the brain-cavity. This seems to indicate that the fibre in life is under considerable tension. My colleague, Mr. Nicholls, who has for some time past been working at Reissner's fibre in this laboratory, will, I hope, bring forward some further evidence on this head very shortly.

I now wish to suggest, for the consideration of physiologists, that Reissner's fibre and the epithelium of the ependymal groove may form part of an apparatus for regulating flexure of the body. Any such flexure would, it appears to me, tend to alter the tension of Reissner's fibre, and thereby exert a mechanical stimulus upon the cells of the ependymal groove to which it is attached. We

may suppose that the stimulus received by these sensory cells is transmitted to appropriate nerve-cells in the brain, and that the deviations of the long axis of the body from the normal position may possibly be regulated by reflex action. We may compare the function of the semicircular canals of vertebrates; also that of the "statocysts" of many invertebrates, which serve, by means of mechanical stimuli due to the action of gravity, automatically to regulate the orientation of the body.

The position of Reissner's fibre, entirely enclosed within the brain and spinal cord, renders it extremely difficult to perform any experiments to test the truth of this hypothesis, but I hope that the ingenuity of experimental physiologists may overcome even such a formidable obstacle as this.

ARTHUR DENDY.

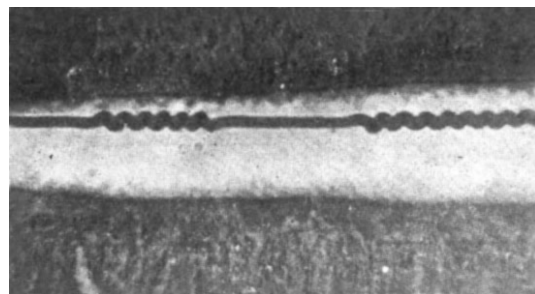
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WITH reference to Prof. Dendy's remarks on Reissner's fibre and the suggestion that he puts forward as to its function, I may perhaps be permitted to add a few words.

Of the preformed nature and of the universal occurrence of the fibre throughout the vertebrate series there can remain, I think, no possible doubt.

During the past two years in which I have been engaged in investigating this structure I have examined sections of the brains of a large number of specimens of more than five-and-twenty different genera taken at random from all the great classes of vertebrates, and in *no case* have I failed to find the fibre in properly fixed material. Any of the commonly employed fixing reagents may be used, but it is essential that the material shall be fixed immediately after death.

I have been greatly impressed with the extraordinary elasticity of the fibre, which in life appears normally to be under considerable tension, so that if it be severed in



Photomicrograph of a section of the terminal portion of the spinal cord of the lamprey. $\times 600$.

the fresh state the free ends recoil sharply. In one case, where in removing the brain of a toad the fibre was accidentally snapped, so considerable was this recoil that, as subsequent examination of the sections revealed, the free end of the fibre sprang forward into the cavity of the fore-brain, the thin roof of which it actually perforated. Another striking instance of this same character is furnished in certain sections of the terminal portion of the spinal cord of the lamprey, a photomicrograph of which, kindly taken for me by Mr. R. W. H. Row, is here reproduced.

In this case the cord was severed when fixation was only partially effected, and a sudden recoil having been prevented by the partial fixation, the fibre was withdrawn backwards towards its attached end by a more gradual retraction that resulted in the production of a number (twenty-nine) of close coils, two of which appear in the photograph.

From these and other facts I am persuaded that the structure is *not* a nerve, but rather an elastic rod with a merely mechanical function, and to my mind the sugges-

tion Prof. Dendy puts forward as to its function is quite a reasonably probable one.

GEO. E. NICHOLLS.

King's College, December 17.

Nitrogen-fixing Bacteria and Non-Leguminous Plants.

I REGRET that owing to Mr. A. D. Hall's letter of July 12 (*NATURE*, July 22) appearing during vacation, I had not an opportunity of seeing it until my return to college at the commencement of term. I trust that, in spite of the long delay, I may yet be allowed to reply to some of Mr. Hall's questions.

Mr. Hall commences by stating that my conclusions are based "on experiments to show that *Pseudomonas*, the bacterium associated with the leguminous plants, will fix more nitrogen, &c." This is not correct, for it is definitely stated in the abstract that both the *Pseudomonas* and the *Azotobacter* used for the nitrogen determination results quoted were obtained from a non-leguminous plant—the root tubercles of *Cycas*.

There are four non-leguminous plants possessing root tubercles which contain nitrogen-fixing organisms. In all four a species or variety of *Pseudomonas* is present, but in *Cycas* only is *Pseudomonas* found living outside the cortical cells, in the algal zone, and in *Cycas* only is *Pseudomonas* found in association with *Azotobacter*. As stated in my paper, the nitrogen determinations quoted had reference only to these organisms from *Cycas*, and were made in order to determine to what extent, if any, *Cycas Azotobacter* assisted *Cycas Pseudomonas* in assimilating free nitrogen.

"*Pseudomonas*, the bacterium associated with the leguminous plants," and the use of "a reasonably active culture" of *Azotobacter*, to quote Mr. Hall, had nothing to do with the determination experiments. On what grounds, then, does Mr. Hall state that "the only conclusion that could be drawn from Prof. Bottomley's figures would be that *Pseudomonas* injuriously affects the power of *Azotobacter* to fix nitrogen"? Naturally, a determination of the amount of nitrogen fixed by the *Azotobacter* alone was made (for Mr. Hall's information I may say that it was very small, -0.56 mgr.), but as the immediate object of the experiment was to determine the effect of *Cycas Azotobacter* on *Cycas Pseudomonas*, it was not thought necessary to quote the figures for *Cycas Azotobacter* alone, especially as this will be dealt with in a future paper.

Again, when speaking of the experiments with oats, Mr. Hall says that the mean error of ± 10 per cent. found in Rothamsted experiments "would more than cover the differences observed by Prof. Bottomley's experiment with oats." The figures given for oats are:—average weight per plant, untreated, 0.42 gm.; treated, 0.74 gm.; increase, 0.32 gm., or 76 per cent. Surely Mr. Hall cannot have read the abstract carefully or he would not have stated that a mean error of ± 10 per cent. more than covers an increase of 76 per cent.! As regards probable experimental error, the abstract states that the oats were "grown in sand, dressed with phosphates, potash and lime." Why, then, should Mr. Hall assume that they were grown "in soil which presumably already contains both organisms"?

As regards the field experiment with barley, duplicate samples were not taken from different parts of the untreated plot, but the total yields from treated and untreated plots were kept separate, and the treated showed an increase of 13.6 per cent. The sample for estimation of nitrogen content was taken from the bulk in each case.

The treated bulbs gave an increased yield of 18.6 per cent. They were not selected as being specially "suitable for experiments on nutrition," but to determine the effect of the mixed culture of bacteria on a totally different kind of plant from any of the other experiments. The bulbs were already planted when the experiment commenced, hence the original weight of the bulbs is not available. As, however, the bed contained 500 bulbs, and was divided

into two equal halves with 250 bulbs in each, one may assume that the original weight in each half was approximately equal.

As regards the culture solution "being a considerable factor in any beneficial effect experienced," it is difficult to comprehend how $4\frac{1}{2}$ grs. potassium phosphate and $\frac{1}{2}$ gr. magnesium sulphate dissolved in a gallon of water for the culture solution, and this afterwards diluted with fifty gallons of water before applying, could possibly produce any appreciable effect on growing crops. Assuming that the bacteria had not used up any of the culture salts in their growth to produce the culture solution, can one imagine any benefit to plants by watering them with a solution containing $1/50,000$ part of potassium phosphate and $1/450,000$ part magnesium sulphate?

Mr. Hall appears to consider his questions "somewhat critical." True and fair criticism is always welcome in scientific investigation, for it is only thus that one can get at the truth of things, but misstatements of fact or misrepresentations of results exceed the bounds of criticism. So far as Mr. Hall correctly states the experiments and results given, one welcomes his criticisms.

W. B. BOTTOMLEY.

King's College, Strand, W.C.,
November 25.

IN Prof. Bottomley's paper in the Proceedings of the Royal Society I cannot read that he makes any claim that the *Pseudomonas* and the *Azotobacter* he isolated from *Cycas* were in any way different in kind from the usual forms of these organisms. When I used the expression "*Pseudomonas*, the bacterium associated with leguminous plants," it was as a sort of explanatory label for the lay reader, but if it has confused the issue at all I would still repeat my former criticism on Prof. Bottomley's basis that the *Pseudomonas* and *Azotobacter* he used are special and unlike all others. The question was if "the association gave an increased power of assimilating free nitrogen," and Prof. Bottomley's figures are:—

Control...	...	...	0.48	mgm. nitrogen
<i>Pseudomonas</i> alone	...	...	0.91	" "
<i>Pseudomonas</i> + <i>Azotobacter</i>	...	...	1.24	" "

and I asked if the action of *Azotobacter* alone ought not also to be known before any answer is possible. Prof. Bottomley now tells us that *Azotobacter* alone fixes 0.56 mgm., so that I still conclude that *Pseudomonas* and *Azotobacter* together (1.24) are less effective than when grown separately ($0.91 + 0.56$), were I not more inclined to think that all the differences are within the limits of experimental error.

To pass to the experiments with plants, by an error which the context rendered sufficiently obvious I wrote "oats" instead of barley when dealing with Prof. Bottomley's first-quoted experiment with soil. The oat experiment is beside the point; it only demonstrates fixation of nitrogen by *Azotobacter* and *Pseudomonas*—a fact on which we are all agreed; but when Prof. Bottomley claims increased crop production due to inoculation with the two organisms in the open ground, we do want the means of judging what weight to attach to the results. He gives for the first barley experiment a gain of 13.6 per cent.; in barley 2 the increase cannot be estimated; the Galtonia experiment shows 18.6 per cent. increase, the parsnips 21.7 per cent. Such percentage differences are not outside the limits of error for a single plot experiment worked on a large scale with every condition in favour of accuracy; when they are differences between lots of 250 bulbs or 68 parsnips I have no hesitation in regarding them as without any significance whatever. I have just had pulled and weighed two lots of fifty successive roots of mangolds growing side by side in neighbouring rows on the same plot, inside rows perfectly similar to the eye and on a very uniform plot, yet one lot weighed 220 lb., the other lot 176 lb., a difference of 25 per cent. The assistant who gathered the roots was quite unaware of the question involved; his instructions were to begin inside the plot and take fifty consecutive roots along a row, then another fifty